Hope, and had there secured and brought home the most astounding amount of both useful and even super-excellent astronomical observations that ever one man made in twelve months,—there was no apparent reason why he should not at once have been allowed to step straight into this new Observatory appointment, and commence its laborious duties forthwith.

But that was not to be; for he was privately informed that he must first and preliminarily be appointed a Professor in the University of Edinburgh. He started at and resisted the idea; he said he did not want to be a Professor, and would not be one; it was an occupation wholly foreign to his tastes, and entirely incompatible with the full and conscientious devotion of himself to being a working astronomer within the Observatory. Pressure, however, of powerful friends was brought to bear upon him; and he was made to understand that Government could not, or would not, whatever the secret reason, create and set agoing the new appointment for the Observatory on the Calton Hill, of "Astronomer Royal for Scotland," without first connecting it with a certain old and shameful sinecure in the University of Edinburgh, called the Professorship of Practical Astronomy.

He was indeed assured that he would, and should, never be called on to lecture in the Professorship; that it was a mere name and nothing more; and his form of appointment to the strangely and unnaturally duplex post of the old Professorship and the new Astronomer Royalship, was made out in words assigning clearly enough the work in the Observatory, of "with zeal and diligence making observations for the extension and improvement of astronomy, geography, and navigation, and other branches of science connected therewith," to be his only circle of duties and his only claim to salary, viz. 300*l.* per annum. But then how were those promises fulfilled; or rather, how were they neglected and overborne when the multitudinous heads of the great educational University of Edinburgh had once got poor Thomas Henderson, the first Astronomer Royal for Scotland, safe within their thrall as being also a Professor before them?

Why thus: they immediately began treading on his toes from every side; and with the most magnificent disregard that he had anything worthy of notice to do in the Observatory, they forced on his attention, both in season and out of it, "that while they were working so hard in the great educational hive, he was a mere drone, and yet was in receipt of a salary of 300*l.* per annum, an absolutely larger salary than any of themselves who bore the brunt and burden of the tuition of all the students." For the complainers, be it remarked, left out of view, that if their incomes did not mount up to 300*l.* in the shape of *salaries*, it was because they came to them chiefly in the form of students' fees; and in that phase sometimes reached 1,000*l.*, 1,300*l.*, and even 1,600*l.* per annum.

But this difference the teaching Professors could not see; and so, if, as they knew perfectly well, there were no students applying for Practical Astronomy Lectures, they determined that the Practical Astronomy Professor should still be educationally utilised, and as an assistant to other Professors, if not as a Professor on his own account. Wherefore Thomas Henderson was talked at, and talked at, until for one winter he was prevailed on to give lectures in the University to the Mathematical Class during the illness of its Professor. Then he was induced to take up the onerous position of Secretary to certain University trusts. And then, while that was still going on, he was over-persuaded into giving lectures for the then Professor of Natural Philosophy during one of his retirements; and then,—why, then, Thomas Henderson, who was all this time struggling almost superhumanly by night and by day to keep up his observations as Astronomer Royal for Scotland in the Royal Observatory, Edinburgh,—why, then—he died! Died too at the early age of forty-six years, and Scotland has not seen his like either before or since; for he was in fact the one and only high-class and complete practical astronomer whom his country and his nation have ever produced; and yet he was hurried to a premature grave, trampled on by an unsympathising *educational* University.

Of my own troubles in trying to fill this truly great man's place, I could tell a vast deal, but would rather merely refer to my last official Report to the Government-appointed Board of Visitors of the Royal Observatory, Edinburgh; wherein, after showing forth the recent attempts of the University authorities actually to "transfer" from the Astronomer Royalship of Scotland the *whole* of the salary originally appointed to that office by the Crown, and take it over to their own studentless Professorship, I have finally besought the Board to apply to Government to separate the two offices absolutely and for ever.

Most heartily convinced therefore must I be of the positive wisdom of those weighty words of Colonel Strange, alluded to in your p. 429; wherein, after stating his belief that there should be a Minister of Science, to look after the interests of institutions for *the promotion of science*, as entirely apart from any or all the institutions for *education*, whether in science or anything else, that most sage and experienced officer goes on to say:—

"That he considers education to be quite a different thing from national research, and that they should be kept as distinct as possible; and that one great evil now existing is the mixing up of those two things."

PIAZZI SMYTH,
Professor, and Astronomer Royal for Scotland
Royal Observatory, Edinburgh

Ericsson's Researches on the Sun

IN your interesting journal (vol. xii. p. 519) I see a description of an experiment by Capt. Ericsson, intended to measure the difference of temperature between the centre and the edge of the sun. I do not intend to make here any criticism on this experiment, but only to make some remarks on the final conclusions.

We must first distinguish two kinds of results—one directly given by the observation, the other by calculation.

In the first, we agree as far as is possible in considering the different methods of solving this question. He finds, indeed, that the intensity near the edge is 0·638 of that of the centre, the outer zone being in the mean line 49″ distant from the edge, and consequently large, 98″ = 1′ 38″. On my experimenting on a small area not exceeding one minute square, and distant from the centre 14′·920 = 14′ 55″·2 (and consequently distant in September from the edge 62″), I found 0·5586. The difference is indeed not very considerable, being 0·0794. Now Plana has shown, in the *Astron. Nachrichten*, No. 813, that such a small difference may lead to a very considerable difference in the value of the absorption.

The value of the solar atmospherical absorption, according to Mr. Ericsson, cannot be greater than 0·144 of the radiant heat emanating from the photosphere (page 520), and he then quotes my results, in which it is stated that 0·88 is absorbed by the whole atmosphere. He proceeds to remark: "It is unnecessary to criticise these figures presented by the Roman astronomer, as a cursory inspection of our table and diagrams is sufficient to show the fallacy of his computations."

I beg leave to observe that the fallacies are not only my own, but those of Laplace and Plana as well, who from the numbers of Bouguer's have arrived at a conclusion very similar to my own. The fallacy, I think, is rather in Mr. Ericsson's method of calculating. In a problem of so great difficulty, and where the great analysts have established very complicated formulæ, he makes use only of some very simple proportions, which are by no means justified, and with these he thinks his conclusion is very plain! I regret to say that such a method of computing in this case cannot be admitted, and consequently we are justified in attributing the difference of the results, not to the fallacy of our computation, but to the fallacy of those proportions assumed by Mr. Ericsson, unless he, or any competent mathematician, be able to show some great error in the formulæ of Laplace and of Plana.

Several objections besides may be made to his manner of experimenting, but of that on another occasion. In applying the numbers of Mr. Ericsson to the formulæ of Laplace and Plana, the result will be found to be not very different from mine. But at present I have no time to discuss these and other calculations, and also I wait for the new experiments which he has promised. I will only add that I do not share his opinion that the lenses and telescopes introduced in these researches by me do not give reliable results.

P. A. SECCHI,
Rome, Oct. 28 Director of the Roman Observatory

Sir G. B. Airy and the National Standards

IN NATURE vol. xiii. p. 35, the following statement occurs:—

"In the civic speeches which accompanied the ceremony [of conferring the Freedom of the City of London], great stress was laid on Sir G. B. Airy's services in connection with the Metric Standards."

This statement is not perfectly correct.

The expression of the Chamberlain of the Corporation, as recorded in the official register, and as correctly reported in the principal newspapers, was:—

"When the national standards of measure and ponderosity were by accident lost to the nation, you were applied to for the accomplishment of their restoration with that mathematical exactitude which was indispensable."

The statement in NATURE will be made correct by erasing the word "Metric" and substituting "National."

G. B. AIRY

The Origin of our Numerals

MR. DONISTHORPE'S ingenious construction of our numerals by corresponding numbers of lines (NATURE, vol. xii. p. 476) induces me to offer a few remarks on this subject, which has a literature of its own. There can be no doubt, I believe, that our forms were derived directly from the Arab series called Gobar; that the Arabs had them from the Indians, and the Indians from the Chinese. My esteemed friend Dr. Wilson, of Bombay, published a "Note on the Origin of the Units of the Indian and European numerals," in 1858,* in which he showed the derivation of some of our numerals from ancient Indian forms found on cave inscriptions of Western India, on the Bhilsa Topes, and on coins. My remarks are founded wholly on the forms given in this note, which is little known, I believe, in England.

Dr. Wilson obtains our first four numeral forms from the Chinese, traced through different Indian script characters nearly as supposed by Mr. Donisthorpe. One, two, three horizontal bars and a square for 4. He also finds the eight in the forms  ,  , and  on the cave inscriptions.

Before proceeding to the other numerals I wish to notice a rule which may be deduced from the consideration of the changes in the forms of numerals in passing from one people to another, that the same form may be turned through angles of 90° or 180°, and may be inverted or reversed without altering its value. Even the same people have used a form turned in different ways for the same numeral. The Arabs used their 2, 3, and 4 in two ways, making angles of 90° with each other; the 2, 4, and 5 of Sacro Bosco and Roger Bacon were the Indian script Modi (and ours) turned through 180°, or upside down; other examples will be noticed.

The most important derivation by Dr. Wilson is that from the Chinese  ten; this is found on the Bhilsa Topes with a circle round it (Dr. Wilson thinks to distinguish it from the oldest form of K found on the cave inscriptions). The nine is found on the Bhilsa Topes as  , or one under ten, and on old coins thus:  . The Indian caves give half of ten  ,  , for five (as V is the half of the Roman ten, X). It is from this form that Dr. Wilson derives the Indian Modi and Nagari fives  ,  ,  . It is here that I venture to differ slightly from Dr. Wilson. One of the cave forms of four is  , which Dr. Wilson interprets (as in the case of nine) one under five, or five less one; now this form without the under bar, as well as the other forms of five, are, it seems to me, the halves not of the cross () merely, but of the cross and circle thus:  , which are as nearly as possible two half diameters and half circumference. The form  is, I believe, the origin of our four, and not the Chinese or Indian square, as supposed. This I think will be evident when we compare the Arab four () with the Indian four above. The Arab four s also employed thus:  , which inverted gives  , a sufficiently near approximation to our four.

Dr. Wilson has not been able to find the origin of our seven, but this is obtained from his Arab seven  , by turning it round () and making one leg shorter than the other, nearly resembling the Gobar seven  . We may also find an earlier source in the Chinese seven turned round 180°,  , which is almost exactly the German written seven. Neither six nor seven is to be found on the cave inscriptions. In Dr. Wilson's Arab series the Indian five is used for six, and the Gobar six, as well as ours, may be taken from the Nagari seven  . We may also find an origin in the Chinese six  , by omitting the horizontal bar, as in the case of the seven. That such liberties were taken is evident on a consideration of the five of Sacro Bosco and Roger Bacon (), the Indian five *without the bar*, and turned round 180°. If there is any merit in these suggestions it belongs to Dr. Wilson.

JOHN ALLAN BROUN

* See "India Three Thousand Years Ago." By John Wilson, D.D., F.R.S. (Bombay; Smith, Elder, and Co., 1858.)

On the Cup-shaped Joints in Prismatic Basalt

THE difference between Mr. Mallet (NATURE, vol. xiii. p. 7) and myself is simply this. He asserts, as necessary to his theory, that the "convexities" should always project in the direction in which the cooling and consequent "splitting is proceeding" ("Proceedings of the Royal Society," No. 158, p. 182). I referred him to the beautiful specimen, in the hall of the Geological Society's Museum, of three columns, one of which exhibits an articulation in the shape of a double-concave lens; the adjacent convexities consequently pointing, in this case, in *opposite directions*.

Mr. Mallet's reply to this is, that the cooling must have proceeded, in this instance, in different directions, and met in the biconcave-lens-shaped articulation. Now, inasmuch as this articulation is only a few inches (three or four) thick, and shows no sign of seam or separation across it, and Mr. Mallet himself declares (in the article mentioned above) that the plane which separates the part cooled from above, from that which cooled from below, "consists of irregular fragments," I maintain that his explanation is inadmissible and self-contradictory. Any geologist who takes sufficient interest in the question to examine the columns for himself will be easily satisfied on this point.

Nov. 8 G. P. SCROPE

A New Palmistry

THE proportions of the fingers in the two hands are not, I think, always the same. With me the index finger of the left hand is considerably longer than the ring; in the right they are very nearly equal.

R. A. PRYOR

Hatfield, Nov. 12

OUR ASTRONOMICAL COLUMN

THE MINOR PLANETS.—The discovery of No. 154 by M. Prosper Henry at the Observatory of Paris, on November 6th, is announced in M. Leverrier's Bulletin and by circular with the "Astronomische Nachrichten;" and that of No. 155 by Herr Palisa at Pola on the 8th inst., in the Paris Bulletin of the 13th. They are of the same magnitude (twelfth) as the three previously detected during the present month.

The rapid increase in the number of small planets must soon occasion serious difficulty, not only in predicting their positions with sufficient approximation to allow of their being recognised without considerable expenditure of time and trouble, but likewise in securing observations, especially on the meridian, according to the system pursued for some years past at Greenwich and Paris, by agreement between the Astronomer Royal and M. Leverrier.

As regards the preparation of ephemerides, it is well known that the conductor of the "Berliner Astronomisches Jahrbuch," Prof. Tietjen, makes it a speciality of his work, with the aid of a numerous body of astronomers in various parts of Europe and in the United States, and hitherto he has succeeded in providing observers with an ephemeris of nearly every small planet detected to within a short time of publication. Thus, in the Jahrbuch for